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# Revisiting ethnography by proxy

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## ABSTRACT

Prompted by discovering a paper written in 1980 which described a process of 'ethnography by proxy', I revisit this concept in the light of two research projects: a workplace study of an electronics company conducted in the early 1990s and a later home-based study of young children's encounters with toys and technology. The paper defines ethnography by proxy as the process of delegating some of the ethnographer's activities to participants in the research setting. It discusses a pragmatic response to some of the challenges of conducting fieldwork and considers the implications of delegating non-academic proxies to fulfil aspects of the ethnographer's role, the different guises that may be taken by the ethnographer and what this shift in research relationships might mean for the interpretation of data. The concept of proxy has implications for some of the tenets of ethnographic research as it requires a re-examination of our roles and the relationship between researcher and researched.

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## The background

In the days before Google, I wrote a research paper about enlisting a manager in an electronics company to collect ethnographic data on my behalf. I described the technique as ethnography by proxy (Plowman, 1996) and thought that I'd coined the expression, as I was not aware of anybody else using it. Fifteen years later, I revisited the idea of conducting ethnography by proxy as a possible approach to exploring children's everyday lives at home. An online search quickly revealed that Wallman, Dhooge, Goldman, and Kosmin (1980) had originated this term many years earlier in their paper *Ethnography by proxy: Strategies for research in the inner city* and this prompted me to consider further the concept of proxy. Usually understood to mean the authority to represent somebody else, in this case the researcher, the word has legal origins meaning an agent or deputy (derived from the word 'procurator', an official of the Roman empire who carried out duties on behalf of the governor or emperor). This sense of deputising, or standing in for, the ethnographer has implications for some of the tenets of ethnographic research and the paper considers the decentring of the role of the ethnographer in two contrasting cases. I propose ethnography by proxy as a pragmatic response to some of the challenges of conducting fieldwork in certain circumstances, suggest that it could lead to a re-examination of the researcher's role, and consider what such a shift in research relationships might mean for the interpretation of data.

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The mediating role of the researcher in collecting and interpreting data is central to many discussions of the foundational elements of ethnography, frequently conceptualised, even in primers, as the ‘researcher as instrument’ (Murchison, 2010, p. 13) or, elsewhere, the self as an instrument of knowing (Dourish, 2014, p. 3). Although Borneman and Hammoudi (2009, p. 14) claim that being there ‘guarantees nothing’, this emphasis on the ethnographer as a filter of experience is reinforced by the widely held view that a prerequisite of ethnography is presence, whether this is ‘going out and getting close’ (Emerson, Fretz, & Shaw, 1995, p. 1) or ‘hanging out and hanging about’ (Woodward, 2008). Geertz (1988, pp. 4, 5) makes reference to this widespread belief when he reminds us that ‘The ability of anthropologists to get us to take what they say seriously ... is a result of their having ... one way or another, truly “been there”’.

Hammersley (2006, p. 4) states that this is usually seen as studying ‘*at first hand* what people say and do in particular contexts’ (original emphasis) but goes on to discuss presence in the context of internet ethnography, for which the data are typically collected online without meeting the participants face to face. Although the two examples on which the following account is based are concerned with technologically-mediated interactions, they take place in real rather than virtual worlds. Nevertheless, Hammersley’s discussion is pertinent because he asks whether the physical presence of the ethnographer among the people being studied is essential, or whether this requirement is premised on an outdated view of ethnographic work.

In *Ethnography by proxy: Strategies for research in the inner city*, Wallman et al. (1980) describe their research goal as an enquiry into the ways in which people of diverse ethnic origins managed the resources of the urban environment that they shared. Some residents in Battersea, an area in inner London, were employed locally to conduct interviews for the project. Wallman’s team used pragmatic considerations as a rationale for this approach, with their intentions being to (i) benefit from interviewers being more knowledgeable about the locale and more likely to ensure a good response rate, (ii) inject cash into the local economy by paying the interviewers, (iii) gain greater access to personal information that might be withheld from strangers perceived as representing officialdom, and (iv) make a saving on travel time. They described two main research strategies in their paper: a neighbourhood survey and a mapping of resource systems. The survey of more than 500 households, which achieved an 85% response rate, established what resources were available to inhabitants and led to identification of a sub-set of families who engaged in the mapping exercise. This led to the production of network diagrams that accounted for variation in how resources (which would probably now be seen as examples of social capital) were used.

Some might doubt whether this approach constitutes ethnography, but the authors argue otherwise. They state that the ‘*ethnographic scope*’ (ibid., p. 25, original emphasis) of the survey was directly attributable to the employment of local residents as interviewers and the ways in which they provided the level of information needed to suggest patterns of social relationships. The authors explained:

We needed research strategies that would reveal – as participant observation might – the ways in which people who come from different places and now live in the same place manage and experience their separate livelihoods, and yet would be feasible – as participant observation is not – in a densely polyglot setting whose residents are united only or most significantly in their distrust of interference from ‘outside’. On both counts we needed research strategies that actively and explicitly involved the people we were studying. (ibid., p. 34)

The two studies in which I was involved shared some of the features of this approach as they both ‘actively and explicitly involved the people we were studying’ and were a response to the potential for distrust of outside ‘interference’. For each study, one in a major engineering company and one based in family homes, I will describe some of the methodological challenges and then provide an account of the ways in which enlisting the concept of ethnography by proxy afforded a working solution. These examples are markedly dissimilar in terms of the location (workplace and home), the role of participants (employees and parents), the purpose of the study (informing system design and discovering more about everyday lives) and the period when they took place (1990s and 2010s). The two examples are used to inform a discussion about the role of the ethnographer, looking at the acceptability and

utility of delegating what are seen as key researcher functions and the different guises that may be adopted by the researcher or their proxies.

### **The engineering company**

The first example is from a workplace study, dating from the mid 1990s, that used ethnographic methods to inform the design of a system to support what is known as computer-supported cooperative work (CSCW). In this research area, tools are typically designed to enable groups of people, whether co-located or distributed, to work together more productively than might be the case without technological support. Workplace studies had become increasingly prominent in the field of CSCW since the late 1980s because work practices were seen as highly situated and so needed bespoke design solutions. Consistent with this, it was considered that ethnographers could study work as it occurred and shed light on the complex interactions that occur in environments such as airport control towers, the London underground and busy offices with the aim of understanding the organisational context in which proposed computer systems would be used (Plowman, Rogers, & Ramage, 1995). As with ethnographic studies in other domains, the methods emphasised natural settings and authentic activities, leading to reliance on observation as a method of data collection, usually supplemented by informal interviews.

The importance of methods associated with ‘being there’ contrasted with the formal task analysis processes common at the time to inform system design. These processes were based on understanding work primarily as an individual cognitive activity, even when the final product was designed for multiple users. Single users were asked to complete standard routine actions in a controlled environment so that information flow and functionality could be determined, typically with a focus on efficiencies. Analysis of their activity was fed into the design process. Conventional CSCW wisdom promoted the view that the consequence of using such techniques could result in end-users needing to adapt their working practices to the software, negating the aim of ensuring that software was designed in such a way that it could be seamlessly integrated into activities.

Those working in CSCW frequently invoked an ethnographic methodology, as a glimpse at the journal *Computer Supported Cooperative Work* (dating from 1992) or the conference proceedings (dating from 1986) of the same name would attest. However, this could be challenging: participant observation of complex work environments can be difficult to authorise and interviews can provide scope for misunderstandings, especially when the subject matter is highly technical. Asking informants to reconstruct practices that draw on tacit knowledge or are semi-invisible to them may lead to an official version of correct procedures in the workplace and a system designed for how things should be done rather than how they are done.

The purpose of the study described here was to support the process of designing and selling complex electronic systems at EquipCo, a pseudonym we used at the time. EquipCo was the third-largest electronics company in the UK and developed systems for commercial, avionics and military use, later becoming a major player in the mobile telephony industry. EquipCo’s location was at a considerable distance from my academic base, so visits had to be pre-arranged, were costly, and infrequent (also a consideration for Wallman’s team in their study in Battersea). Combined with the limited resources made available for social science research in what was essentially a computing project, it was inappropriate to use methods that relied on immersion in a culture over a long period of time. However, it was primarily the perceived risk of damaging EquipCo’s commercial interests that led me to reconceptualise the fieldworker’s role and adapt standard ethnographic approaches.

### **The methodological challenge**

Observation is a key resource for ethnographers but what is observable can be circumscribed in an industrial environment and the researcher may find that they cannot gain access to the key aspects of the work that the proposed equipment is designed to support. As most of the systems sold by EquipCo were bespoke, requirements capture to establish the purpose of the finished product was an essential

part of the sales process. The goal of the project on which I was employed as a postdoctoral researcher was to design and build software, known as COLlaborative REquirements Capture Tool (CORECT), which would run on a laptop and bring together all the different aspects of system design and costing. Hitherto, this process of requirements capture involved a salesperson eliciting information from the customer about their needs and preferences in a face-to-face meeting and rapidly sketching their proposed designs for the electronic system on paper. This was followed at a later stage by engineers and the marketing department drawing up a detailed and costed specification (Rogers, 1995). The purpose of CORECT was to speed up this process, make it more responsive to the client's needs and facilitate communication between the different parties, enabling various actors in the commissioning process to add data and make changes.

From a workplace ethnographer's point of view, the opportunity to accompany salespeople at meetings with potential customers would provide insights into the design of the proposed tool and how it could be used, such as the extent to which the customer would have a shared view of the screen and what information would be available to both parties, given that pricing data remained undisclosed at this stage. But even if EquipCo could be persuaded to allow me to attend meetings, the commercial sensitivities involved meant that they could not authorise such access on behalf of their potential customers and they would not wish to jeopardise the relationship by making such a request. This meant that an essential source of information for understanding the requirements capture and design process was not available.

### **The response**

Unable to persuade EquipCo's managers that I should attend these sales meetings, it was nevertheless agreed that it was worthwhile to collect data on work practices in these initial consultations with the customer as it would enable us to consider the possible impact of introducing a laptop into the scenario. Accordingly, Alan, EquipCo's Senior Marketing Manager attended a training session with me. Alan had high status within the company and he routinely sat in on meetings between potential customers and members of their sales team as part of the staff development programme. Having been involved in the research project's earlier design discussions, he also had a well-informed view of the proposed CORECT system and so was able to visualise it in use in the meetings in which he would be a proxy ethnographer. He planned to write notes in his daybook as usual and then transcribe them for later debriefing sessions.

I asked him to focus on occasions when the paper documents were shared as opposed to used individually, indicate whether these documents were annotated or notes were kept separately, register how the sales staff and potential customers oriented themselves to the shared materials and report on the ways in which design issues or the functions of specific parts were explained. In a debriefing meeting, Alan provided a sketch of the layout of the meeting and listed the participants (a purchasing manager and a planning engineer for the client company, CeeCo, and a systems product manager, a sales engineer and Alan for EquipCo). He had already transcribed a couple of pages of notes and then talked me through the rest of his handwritten notes and sketch diagrams to denote positions of people relative to the shared materials, indicating movement where applicable. He noted eye contact or its absence meticulously and we discussed the potential impact of a laptop computer mediating their communication.

Alan thought that it could be difficult to effect an easy transition between taking freehand notes and using the laptop and that tapping on the keyboard could be disruptive. (This study took place in 1993, before laptop computers were routinely used in meetings and when they were considerably bulkier and more intrusive than they are now.) He preferred to continue with paper-based notes so he could annotate, locate information, and draw technical diagrams easily. Nevertheless, his role was successful inasmuch as the design took account of various aspects of his input and a working prototype to run on a laptop was subsequently developed that included an automatically generated 'to do' list



that guided the user to a correctly constructed design artefact, with prompts for more detail where needed but without overly constraining the options (Rogers, 1995).

## Technology in the home

Many years later (2008–2011), I wondered whether ethnography by proxy might solve a research challenge in a project that aimed to describe three- and four-year-old children's encounters with technology in family homes. As in the CORECT study, we wanted to focus on the mundane features of natural settings, but a traditional ethnographic methodology of participant observation and extended interviews was not suitable. The goal was similar inasmuch as both studies were predicated on a rich description of the environment and the participants' actions; the key differences were in the purposes to which the description was put. The intended outcome for the CORECT project was a functioning computer system that assisted EquipCo in its ambition to conduct the sales process more cost effectively, as befitted a study partly funded by the then UK Department of Trade and Industry. The stated aim of the toys and technology project, funded by the UK Economic and Social Research Council, was to develop a detailed description of family practices around technology with the aim of finding out more about what and how children were learning, the use of domestic space, and parental perceptions of their child's play. On the face of it, this was a more conventional ethnographic project than CORECT as we wanted to describe children's everyday lives and the technological and social landscapes in which they lived rather than to evaluate or design specific products.

The study focused on 14 households in central Scotland, with half the families categorised as being of low socioeconomic status in terms of the parents' employment and education. All of these families maintained their involvement throughout the duration of the study, enabling us to trace children's encounters with toys and technologies at home over the course of nine or so home visits. The preschool years are a time of rapid change in children's lives and we drew on a range of methods to enable us to document and examine the complex interactions with peers, family members, the technology, other toys and cultural practices during this period. Each visit had a specific focus, such as surveys of the toys and technologies available to the children, 'toy tours' of the home involving children photographing and talking about their favourite toys, parental perceptions of their child's play and learning, video recordings of children's interactions with technological toys and family interviews on the changes brought about by the transition from kindergarten to school at the age of five. The emphasis on typically occurring activities provided underpinning for an ethnographic approach to the study of technology, play and learning at the intersections of cultural context and individual variability of child and family.

## The methodological challenge

As described above, asking the sales and marketing people at EquipCo to describe their work practices was of limited value. Similarly, interviews with parents were not very revealing about their everyday practices as it was difficult for them to formulate insights into what they saw as routine parenting. For example, when asked how their children had found out how to interact with various technologies, parents expressed the belief that their child's competencies were mainly the result of being self-taught and that they 'just picked it up'. Parents were oblivious of the ways in which they supported their child's learning through their own uses of technology – by modelling behaviours or by providing opportunities to participate in and observe authentic activities – and so they found it difficult to provide descriptions of their own practices in conversations with us (Plowman, McPake, & Stephen, 2008).

The lack of prior studies that we could draw on to inform the design of our study meant that there was an absence of ready-made methods. In this regard, notwithstanding the lapse of more than two decades, there were some similarities with Silverstone, Hirsch and Morley's account of an ethnographic approach to the study of technologies in the home. They refer to conducting research 'within the private worlds of those within one's own culture' and working with families to understand their relationship to these technologies as 'an intensely problematic activity' (1991, p. 205). Their use

of standard procedures such as field notes, interviews and time-use diaries revealed the ‘irresolvable limitations of a participant observational ethnographic approach’ (ibid., p. 210).

Our fieldwork revealed some of the conditions and objects that act as triggers to or constraints on a child’s play and learning with technology. However, in trying to explore the reciprocal influences between the various factors that permeated family life we faced four key methodological challenges: (i) families may resist extended observational research by outsiders in their homes; (ii) our home visits generally took place during working hours, so we wanted to know more about what happened in our absence and what children were doing beyond the home; (iii) the three- and four-year-old children were not able to remember some of the activities they had been involved in, or to describe them fully, and (iv) we wanted to address the risk that our interrogation could alter the very practices in which we were interested.

### **The response**

The restrictions meant that we needed to develop an approach that did not depend upon the researcher’s presence at all times. Our solution was to ask parents to become proxy ethnographers by using their own mobile phones to create a visual diary of family activities. We sent text prompts six times at intervals between 09.00 and 17.00 on the pre-arranged days and parents were asked to respond with a picture message of their child along with text stating their location, who they were with, and what they were doing. We completed three rounds over a period of several months with the eleven parent-photographers, all on Saturdays.

As well as providing insights into the spatial dimensions of children’s lives, the automatically date- and time-stamped messages created a record that included the mundane or routine aspects of day-to-day life that can get overlooked by interviews. Combined with the other approaches listed above, the method enabled us to illuminate the range of activities, resources, people and places that constitute the home and are difficult to access by other means. The images and text messages were collated to form a storyboard that served the dual purpose of being an early-stage analytical process that tracked the temporal shape of the days and a visual diary that formed a stimulus for discussions with parents and children. (For an example, see Plowman and Stevenson, 2012. Phoenix and Brannen, 2014, describe alternative approaches to the use of photographs to prompt narratives of everyday life.)

We wanted to develop our understanding of play but we had found that asking parents direct questions about their definitions of play in an interview was, not surprisingly, problematic. For this exercise, parents had not been asked to focus on play but on ‘what a weekend day involves for your child’. We were able to isolate all references to play in the text messages and were able to establish that children were perceived to be playing across a wide range of activities and at different times of day. There were considerably more play episodes in the afternoon, as domestic chores were completed in the morning to leave the afternoon free for more child-centred activities, but the rest of the time – on these Saturdays, at least – was spent eating, napping, shopping and cooking, or going on outings with the child’s entertainment and enjoyment in mind. The deployment of parents as proxy ethnographers helped us to get a better sense of the range of activities in a child’s day and contributed to our understanding of how parents resourced and supported play by ensuring a balanced range of activities and involving children in the intergenerational practices that suffused family life.

Parents were willing proxy ethnographers by this stage as we had proposed the activity after several home visits. The 96% response rate suggested that the method was not overly complex or onerous, even though diary studies usually require a high level of commitment. In the same way that it was entirely usual behaviour for Alan to record details of sales meetings in his daybook, there was nothing about the act of taking photos and using a mobile phone that was unusual in the home setting. The photos taken for the mobile phone diaries straddled the personal and the documentary: for the mothers who took the photos, they were images of their child, their home, and their possessions; for us, they had ethnographic value, enabling us to gain access to the intimate setting of the home without being disruptive or invasive.



## The role of the ethnographer

Whether describing remote or local cultures there is a long history of examining the role of the ethnographer (e.g. Agar, 1980; Drake & Harvey, 2014; Hobbs & May, 1993; Jones, 1970) but this has been so bound up with the centrality of participant observation that there has been very limited acknowledgement of situations in which one might want to consider delegating the role. Conventionally, the fieldworker has maintained a professional detachment and relied on one-way communication from the informants that is then transformed by a mystical and opaque process into ‘findings’ by the researcher. Other aspects of what Breglia (2009, p. 131) calls the Traditional Fieldwork Model and its basic guidelines for ‘being there’ have been questioned, but it has seemed almost treacherous to consider enlisting the help of proxy ethnographers.

### *Delegation and utility*

In some respects, ethnography by proxy is not so different from the ways in which collecting qualitative data from the field is routinely delegated to research assistants. In their report of studies in Mozambique and Tanzania, Deane and Stevano (2016) point out the value of locally employed research assistants when the principal investigators are not familiar with the environment or indigenous languages. James (2013) discusses cases where grant-holders are not involved in primary data collection, although she focuses more on the ways in which the constraints on funding mean that a grant-holder’s allocation of time may be limited to project management and oversight of a team. She asks:

What happens, then, when the researcher is not there, when their engagement with ‘the field’ is potentially reduced to shuffling pieces of paper as, in the office, they read through transcripts of interviews done by others on their behalf? Are the invaluable insights gained from immersion in the field or reflexive readings of data simply lost to the potential detriment of the research? (James, 2013, p. 565)

In their analysis of how government policy can determine research activities, Mills and Ratcliffe (2012, p. 152) explore the ‘methodological instrumentalism’ by which ethnographic practices have been redefined by the expectations and values of a knowledge economy and the associated growth in the monitoring and assessment of research. They describe (ibid., p. 158) how, as a partial consequence of changes in the funding regimes, ethnography has found a place in the business sector. This has led to the growth of corporate ethnography (a term that could be applied to the EquipCo study) and ‘a trend towards short-term studies, whose findings can be easily used by those – be they bankers, investigative journalists, financial analysts or management consultants – whose work benefits from an ethnographic sensibility’. The reduction in the time typically spent in the field is also touched on by Hammersley (2006, p. 5), although he points to the increasing use of portable recording devices as a factor, given the ease with which data can be amassed now.

In the UK, demands for greater research productivity have made extended periods of fieldwork a rarity, even though this is still considered by scholars to be a key metric of quality. The pressures of performativity and utility, the requirement to demonstrate value for money and societal impact, and the increased competition for research grants converge to determine what is assessed as fundable research. These conditions are not new. Wallman et al.’s (1980) rationale for ethnography by proxy included practical considerations such as travel costs as well as mentioning that an anthropological interest in urban settings in home countries rather than overseas came about partly as the result of limited research funding. The CORECT project also foreshadowed recent edicts in the UK relating to impact and engagement with research users, prompting the comment that research initiatives often required an industrial partner (Plowman et al., 1995, p. 312). In that example, the very purpose of the ethnographer’s findings was to inform the design of a system that would have direct impact on the organisation being studied, unlike those who become involved in other cultures with a less interventionist stance. However, the instigation of change is not unusual for ethnographers in fields such as education, housing and health care. Wallman et al. (1980) were well aware of the policy implications of their work; more recently, Adair (2011) has advocated the role of ethnography in informing policy in

early childhood education, Lewis and Russell (2011) describe researchers who are embedded in community health organisations creating what the UK research funding councils call knowledge exchange in their ‘way forward’ for ethnographic research and Teusner (2016) describes an ethnographic study aimed at improving occupational health and safety in business.

Does this mean that the concept of ethnography by proxy is just a capitulation to pressures to be quicker and cheaper, more applied and more impactful? While traditional ethnographies continue to have a place, restrictions on access to sites, to people, or to artefacts are commonplace. The ethnographer has always exercised their professional judgement about balancing utility with potential reward. This is not simply an abdication of the researcher’s responsibility: it’s about finding a way to represent the minutiae of people’s lives that is both pragmatic in its use of resources but also provides richness of data. The fact that it is not based on notes handwritten by the ethnographer during long stints in the field does not invalidate the approach.

### **Whose interpretation?**

Some might claim that delegating the collection of data can lead to a disjunction between the process of gathering data and its interpretation because the person who collected the data does not undertake substantive analysis. But whether the data are network diagrams, as in the Wallman study, notes and sketches from a meeting at EquipCo, or mobile phone picture messages from families, the process of collecting data means that the proxy ethnographers are involved in preliminary analysis. Even if they are not aware of it, the selection of which features to note or record is an analytical process in itself. In the toys and technology study, the parent-photographers provided a first-stage interpretation of their children’s activities through the medium of the text message. Although a limited number of text characters was available and the content of the accompanying text was specified in terms of responding to the research team’s prescribed questions of who, what and where, the parents still made the decision about how to frame the photograph visually and how to describe their child’s activity in words. At a later stage, parents and children were invited to share viewing the storyboard created from the sequence of text and images and provide a commentary on the extent to which it reflected a typical day. This form of photo elicitation provided an interpretation that was valuable for highlighting the features of the day that were memorable for the young children.

When Alan drew the layout of the room in which the EquipCo sales pitch was taking place he was making decisions about what to include and exclude. He was not able to capture all the discussions that took place but selected the snatches of conversation that seemed pertinent within the restrictions imposed by his active involvement in the sale. During the debriefing he made his own suggestions about ways of interpreting material and answered the questions I posed based on a research-informed knowledge of some of the likely topics. Wallman et al. (ibid., p. 34) also address this issue by stating that ‘the two strategies outlined in this paper are called *ethnography by proxy* not because informants are involved in collecting and recording ethnographic data, but because they are also involved in its interpretation’ (original emphasis). In all these cases, the local residents in Battersea, the parents in their homes and the manager in the sales meetings, the proxy ethnographer was an expert in their own domain. Their interpretations could be more direct and provide insights unavailable to the researcher.

### **Different guises**

Some of the correspondences between being a sales manager and being an ethnographer are explored by Wasterfors (2008, p. 236), who talks of ‘the missionary, the colonial administrator, the traveler, the tradesman, or the idiosyncratic collector of songs and folklore’ as early examples of amateur ethnographers, describing this as ‘lay ethnography’. His study of how Swedish and Swedish-Polish businessmen worked in the emerging markets of Eastern Central Europe after the collapse of Communism in the 1990s recounts how they played the role of ethnographers in their attempts to get to know a culture and its language so that they could forge business relationships. Like Alan’s, theirs was a pragmatic

form of ethnography, with little of the self-examination and reflexivity associated with academic researchers in the field and a fairly fluid movement between detachment from, and involvement in, the organisations or cultures from which they wished to profit.

The data generated by the proxy ethnographers in the cases described here were designed to be a supplement to other sources, so the preparation they received from the main project investigators was sufficient for the purpose. Alan had an initial training session with me but, as a marketing manager, this was building on his existing powers of observation and his ability to interpret and analyse people's actions in a sales context. Used to attending meetings such as the one with CeeCo, Alan already demonstrated an 'ethnographic curiosity' (Deeb & Marcus, 2011, p. 57) and was probably more effective than I was at decoding interactions between customers and sales staff. Of course, the ethical implications of collecting data in this way are complex. Personnel from CeeCo were unaware that the data from the meeting were being used for design purposes. There was less awareness of ethical sensitivities at the time and this was not injurious to their interests but I would now insist on providing full disclosure of how the meeting interactions would be used and discourage covert activity of this kind. Okely (2007, p. 359) draws an analogy between anthropological fieldwork and espionage and it is easy to imagine how the role of the proxy ethnographer, especially in an industrial setting, could be seen in this way. (In a rather more benign example of an ethnographic study of children's play, Richards (2011, p. 313) describes a boy in a school playground asking if he is a spy.) Indeed, the reason why EquipCo had not wanted me to attend the sales meetings was because avionics equipment was being designed and sold and they were protecting their own, and their clients', interests. As a senior manager, Alan was able to use his own judgement about what was disclosed in the debriefing sessions with me.

An organisation's reluctance to have an outsider scrutinising work practices is also found in Gusterson's (1997) attempt to carry out an ethnography of the Lawrence Livermore National Laboratory for nuclear weapons in California. As he was not allowed into most parts of the laboratory, participant observation was not an option. He describes his solution as 'polymorphous engagement', an approach that engaged with a wide range of sources in different ways across a number of sites, sometimes virtually, claiming that this 'preserves the pragmatic amateurism that has characterized anthropological research, but displaces it away from a fetishistic obsession with participant observation' (ibid., p. 116). Gusterson did not use proxy ethnographers as one of his strategies but his emphasis on pragmatism and amateurism suggests that this may have been a workable strategy. Indeed, if he had been able to recruit a proxy he may have been saved the incident he describes in which one of the weapons scientists, unhappy at the prospect of an ethnographer in their midst, came to a meeting 'wearing nothing but a loincloth and carrying a cane to which he had nailed an animal skull', so emphasising 'the objectifying, exoticizing language of anthropology' (ibid., p. 117).

The parent-photographers operated in a different context, but their role was also an extension of their typical conduct. Indeed, it is more likely to be considered aberrant behaviour if parents do *not* take photographs to record their child's life as they grow up. We had only intended originally to conduct one round of collecting data in this way as we were concerned that it would be onerous for the families but we continued with two more rounds because they enjoyed the process, there was a high degree of task compliance and the data it generated was so rich. For the children concerned, there was nothing unusual about the process, as was the case for the participants in the EquipCo sales meetings.

In the examples outlined here, I have described the local residents, a sales manager and parents as proxy ethnographers. The ethnographer, then, can appear in different guises but there are other ways of conceptualising this role: as 'epistemic partners' (Deeb & Marcus, 2011, p. 64), 'apprentices' (Downey, Dalidowicz, & Mason, 2015) 'confederates' (Silverstein, 2011, p. 77) and 'folk ethnographers' (Wasterfors, 2008, p. 251). We can conjure up others, ranging from the slightly pejorative 'accomplice' to the more neutral 'co-optee'.

## Conclusions: ethnography by proxy

In her contribution to pedagogical reflections on learning anthropology in a period of transition, Breglia (2009) resisted pressures that have led to the diminution of the ethnographer's apprenticeship and regrets the perception that competence requires no more than 'time and patience' and 'a friendly and approachable disposition'. This leads, she says, to others seeing ethnography as 'neither specialized nor professionalized nor technical'. Forsythe (1999, p. 130) referred to what she saw as widely held misconceptions about an ethnographer's desirable skills, principally that 'it's just a matter of common sense' and 'anybody can do it'. She articulated undisguised exasperation in her account of the way ethnography has been utilised for the development of CSCW systems such as the CORECT case described earlier, claiming that so-called ethnographers who conduct workplace studies lack training, produce superficial analyses and make elementary mistakes such as failing to uncover tacit assumptions.

Where some see a reductionist appropriation of ethnographic approaches for instrumental ends, I see a commonsensical response to problems of access and intrusion. Although there have been reappraisals of the centrality or otherwise of participant observation (Feldman, 2011) and participant listening (Forsey, 2010) discussion of the role of proxy ethnographers has been missing. Is this because it is a step too far? Does it mean that we cannot lay claim to authentic ethnography if we turn to proxies to collect data?

There has been considerable soul-searching in an attempt to figure out how ethnographers in the field should relate to the 'other', their informants or participants. These changes have happened in parallel with the shift in how ethnography is conceptualised, including not only the decentring of the fieldworker but also a greater interest in multiple sites in a globalised world. As Marcus (2009, p. 1) puts it, fieldwork projects in anthropology 'are not what they used to be'. Across the three examples outlined here, the urban setting, the engineering company and families at home, we can see that the proxy ethnographer is pivotal in linking the fieldworker to the people who are the 'others' of the research process. This may blur the boundary between 'the professional community of observers and those observed' (Marcus, 2006, p. 116) and enable us to find ways of rebalancing this relationship. Those people that we routinely describe as informants, subjects or participants have a different relationship to the proxy ethnographer: rather than positioned in relation to an academic researcher, they are local residents, colleagues, customers, family members or friends.

As proxy ethnographers they mediate what we can know and how we can know it, with their roles in the co-construction of knowledge and its dissemination contributing to an epistemological shift. I do not discuss here the implications of this shift for the textual form of the ethnography as this has been achieved more eloquently elsewhere (e.g. Marcus, 2012) but it is clear that it opens up the potential for collaborative texts that may be accessible beyond academic audiences. While purists may balk at the prospect of yet another tenet of ethnography being dismantled (to add to recasting thinking in recent decades about the duration of immersion, the otherness of place, the supremacy of observation and the immediacy of relationships), the purpose of revisiting the notion of ethnography by proxy is to think anew about the desirability or feasibility of the academic researcher's role as the lynchpin of the ethnographic endeavour. Ethnography by proxy may serve as a way of making ourselves more accountable to the data and our informants, as well as presenting a pragmatic solution to some of the challenges of conducting ethnography in current conditions.

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